first volume. The names of the writers are a guarantee for the excellence of the work; and, where not wholly rewritten, the articles have been revised and brought up to date. Mr. J. Arthur Thomson is responsible for the zoological articles, which in this volume are Bee (in which Sir John Lubbock has assisted), Bird, and Butterfly; Mr. Patrick Geddes is the writer of the articles on Biology, Botany, Bud; Dr. W. Inglis Clark writes on Carbon, Prof. James Geikie on the Carboniferous System, Prof. Wm. Thomson on Capillarity, and Mr. A. Fraser on the Calculus. The articles on engineering and architectural subjects are contributed by Messrs. D. and T. Stevenson, D. K. Clark, and David MacGibbon. In geography, Sir Charles Warren writes on Bechuanaland, Sir Charles Bernard on Burmah, Mr. S. Lane-Poole on Cairo, Mr. Macdonald, of the *Englishman*, on Calcutta, Prince Kropotkine on the Caspian Sea. Amongst other geographical articles are those on Belfast by Mr. T. Macknight, Birmingham by Mr. S. Barnes, Bolivia by Mr. W. Dundus Walker, Bristol by the Rev. W. Hunt, Brittany by Mr. Thos. Davidson, Bulgaria by Mr. A. Silva White, Cambridge by Mr. G. H. Smith, Canada by Mr. J. G. Colmer, C.M.G., Cape Colony by the Rev. J. Mackenzie, and Cashmere by Major Holdich. Five excellent maps accompany this volume—namely, (1) Belgium; (2) Burmah, Siam, and Assam; (3) Canada, Eastern Provinces; (4) Canada, Dominion of; (5) Cape Colony and South Africa. The less important articles are also very satisfactory. For those who desire further information on the various subjects a list of authorities is given. Many of the articles are models of compression. The article on Carlyle is an instance of this. Here the large and growing literature relating to Carlyle, published since his death, is compressed into the space available in a manner that is little short of amazing. In this and one or two articles which we have noticed, the very difficult art of saying much in a little space, of reducing volumes to paragraphs, and even to lines, is exhibited in a high degree of excellence.

Star Atlas, containing Maps of all Stars from 1 *to* 6·5 *Magnitude between the North Pole and* 34° *South Declination, and all Nebulæ and Star-clusters in the same Region, which are visible in Telescopes of moderate powers, with Explanatory Text.* By Dr. Hermann J. Klein. Translated and adapted for English readers by Edmund M'Clure, M.A., M.R.I.A. (London: Society for Promoting Christian Knowledge, 1888.)

THIS is a most important addition to the stock-in trade of the amateur astronomer. The eighteen maps, printed by Funke, of Leipzig, are as clear as they can be, the letters and constellation boundaries being given in red ink.

There are some useful tables given in the introduction, and these are followed by a catalogue of the most interesting objects, which seem to have been very carefully chosen by a practical astronomer, and the editor has done his best to bring the accompanying notes down to the latest date.

In addition to the maps, some excellent illustrations of clusters and nebulæ are given, and no pains have been spared to give as much useful and trustworthy information as possible.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Alpine Haze.

FOR some years past, occurrences in the Alpine atmosphere have frequently reminded me of effects produced in the "experimental tubes" with which I worked some years ago. When the experimental tube was already occupied by a fine "actinic cloud," it was a common experiment to precipitate within the tube an ordinary cloud by dilatation. The melting away of this latter, and the reappearance of the finer and more persistent cloud, which it had for a short time disguised, were curious and instructive effects.

In the valleys of the Alps floats, not unfrequently, a fine haze, much resembling the actinic clouds. This year the haze was more than usually prevalent, being sometimes very curiously distributed. It frequently filled the great Rhone Basin, below Alp Lusgen. Amid the haze, patches of true cloud would appear, extending till they became continuous, and filled the basin. A floor of cloud, usually shining white, would then spread below us. Under a strong sun, the cloud would disappear, leaving the more permanent haze behind. The haze could not have been aqueous. I have seen the dense true cloud disappear early in the morning, and the haze continue through a fervid summer day, until the moon came out at night to illuminate it. The distribution of the haze this year, and the consequent precipitation were often remarkable. Looked at from our eminence, the haze would be seen filling the lower valley, but divided above into long horizontal striæ, which were obviously the edges of haze-layers, foreshortened from our point of view. Mr. Stirling's beautiful observations were frequently brought to mind by the obvious tendency of the aqueous clouds to form in, and to follow, the haze. A highly picturesque distribution of the clouds was often thus produced. What the haze *is* I do not, for a certainty, know; but that it is not water is proved by its persistence in presence of a powerful sun, and above the heated earth-surface. The late Prof. De la Rive would probably have referred it to floating germs (see letter to myself "On the Organic Dust of the Air," *Phil. Mag.*, 1870, vol. xxxix. p. 229). The prevalence of autumn pollen in the air may, perhaps, account for the singularly striking cloud effects invariably observed at Alp Lusgen, at the end of September and the beginning of October.

JOHN TYNDALL.

Hind Head, Haslemere, October 30.

Prophetic Germs.

MY desire in this controversy has been to bring the Duke of Argyll's theory to the test of fact. But I cannot obtain from him any statement of fact which tends to support his belief in prophetic germs. He cites the well-known observation that in the growth of the individual from the egg, organs pass through rudimentary stages, during which they are not used. He then says: "On the Darwinian hypothesis this fact applies equally to the birth of species." Does it? It is not worth while posing opinion against opinion. Let us have some facts. Can the Duke of Argyll, or anyone else, adduce an observation of fact which necessarily leads to the conclusion that a given organ in a given animal or plant has passed through rudimentary stages in ancestral evolution in which that organ's rudiment had no use?

I am inclined to think that there are some cases which might appear to be of this nature, but are to be explained as due to "concomitant variation" or "correlation of growth" in a complex highly-elaborated organism, one part developing, though without use, as the necessary mechanical or structural condition of the development of another part which *has* use.

Such cases will not serve the purpose of establishing a general law. Will the Duke undertake to tell us what were the rudimentary stages of the limbs of Vertebrata in which actual use was impossible? Will he give a similar history of the vertebral column, or of the brain and spinal cord, or of the eye? In short, are there any facts in support of the theory of prophetic germs? Unless such facts are cited, your readers will conclude that the theory of prophetic germs is devoid of basis.

E. RAY LANKESTER.

45 Grove End Road, N.W., October 26.

Mr. Romanes's Paradox.

I SHOULD be sorry to have misrepresented the views of Mr. Romanes, especially on so formal an occasion as a Presidential address at a meeting of the British Association. But, if I have done so, I must plead in extenuation that I know of no recent writer whose papers I find so difficult to thoroughly comprehend. With an appearance of lucidity there seems to me to be often an underlying obscurity of ideas by which I find myself as often completely befogged.

It appears to me that it is sometimes overlooked that what is usually called the "Darwinian theory" is set out in a book which bears as its title the words, not, as they are usually quoted, "The Origin of Species," but "The Origin of Species by Natural Selection." These words I regard as a proposition of which the book itself affords what is intended to be the proof. It seemed to me that Mr. Romanes intended to distinctly traverse this proposition, and, this being so, the careful consideration of his views became a matter of very great importance. Mr. Romanes now denies that he intended anything of the kind. But the denial comes rather late in the day, because the impression which I received from his paper at the Linnean Society was certainly shared at the time by others. For example, though it is unusual for a purely scientific paper to receive an extended notice in large print in the *Times*, Mr. Romanes was so favoured, and here is what the *Times* (August 16, 1886) says on one of the points on which Mr. Romanes complains that I have misrepresented him :—

"The position which Mr. Romanes takes up is the result of his perception, shared by many evolutionists, that the theory of natural selection is not really a theory of the origin of species, but rather a theory of the origin and cumulative development of adaptations." Now, I suppose Mr. Romanes would call this an "absurd misrepresentation." If so, it is singular that, as far as I remember, he took no steps to correct the statement of his views to which the *Times* gave its wide circulation.

But is it a misrepresentation? It is not, I think, difficult to cite a good deal of evidence that it is not. Anyone who will take the trouble to refer to the Journal of the Linnean Society, Zoology, vol. xix. p. 345, will find printed in capital letters across a page of Mr. Romanes's paper, "Natural Selection not a Theory of the Origin of Species." Now, everybody knows that the idea of the evolution of organic nature existed in men's minds long before Mr. Darwin. He did not originate it ; what he did originate was the theory that "natural selection" is the mechanical means by which that evolution has been brought about. Mr. Romanes says roundly that it is not, or words have ceased to have meaning. Well, coming from "the biological investigator upon whom," the *Times* tells us, "in England, the mantle of Mr. Darwin has most conspicuously descended," I thought that a "startling paradox," and I said so. There was nothing very novel in this : it only put into other words what Mr. Wallace had already said (NATURE, vol. xxxiv. p. 467), when he took exception to Mr. Romanes's "extraordinary statement that, during his whole life, Darwin was mistaken in supposing his theory to be 'a theory of the origin of species,' and that all Darwinians who have believed it to be so have blindly fallen into the same error."

The next point on which Mr. Romanes complains is that I make him say specific differences are not adaptive, while those of genera are. And he calls this an absurd misrepresentation ! It is really too comical, because it is the key of his whole strategic position. When Mr. Romanes read his paper at the Linnean Society, he began by saying that he regarded it as the most important work of his life. And the expression would certainly not have been exaggerated if he had succeeded in establishing what he terms (capitals again) the "inutility," *i.e.* non-adaptiveness, "of specific characters." Even Mr. Romanes could not assert that all specific characters are non-adaptive. But he asserts (NATURE, *l.c.*, p. 314) that "a very large proportion, if not the majority, of features which serve to distinguish species from species are features presenting no utilitarian significance." If this could be proved, it would be quite as effective as proving the proposition universally in inflicting a deadly blow on the Darwinian theory, the very essence of which is that specific differences must be advantageous. I agree with Mr. Wallace (NATURE, *l.c.*, p. 467) "that there is no proof worthy of the name that specific characters are frequently useless."

I am of course prepared to admit that, in regard to plants, about which only I feel competent to speak, there are a vast number of specific differences the adaptive significance of which we are either wholly ignorant of, or, at any rate, very imperfectly understand. But Mr. Darwin has himself led the way in a host of discoveries which have shown in innumerable directions, which had never been previously suspected, the adaptive significance of plant structures. We seem to me justified, then, in drawing the conclusion that all specific differences in plants are probably adaptive. This Mr. Romanes calls reasoning in a circle ; to me it seems only a reasonable induction, the validity of which is strengthened every day by fresh observation.

As to the distinction which Mr. Romanes draws between specific and generic differences, I only summed up what he repeats again and again. Here is a specimen :—"It is comparatively seldom that we encounter any difficulty in perceiving the utilitarian significance of generic and family distinctions, while we still more rarely encounter any such difficulty in the case of ordinal and class distinctions. Why, then, should we encounter this difficulty in the case of specific distinctions?" In my opinion the actual state of things is exactly the reverse. But, as I discussed this point at some length in my Bath address, I need not touch upon it further.

I do not undertake to follow Mr. Romanes into all his dialectical subtleties. But the position which I understood him to have taken up in his paper was quite intelligible, and was of very great interest to the biologist. I briefly analyze it as follows :—Mr. Darwin explained the origin of species by natural selection ; this implies that specific differences are adaptive ; but this is not universally the case ; it follows, then, that natural selection is not the explanation of the origin of species except when specific differences are adaptive, which, in point of fact, they are not in the majority of cases. It is clear that this shrivels up the part played by natural selection to very small dimensions, and minimises pretty effectively in proportion the position of the Darwinian theory in the field of biological speculation. The force, however, of the whole train of argument obviously depends, as I have remarked before, on the proof which can be given of the proposition that the majority of specific differences are non-adaptive. When we turn to the part of Mr. Romanes's paper dealing with this vital point, we only find some not very convincing assertions—some of which I think are erroneous —and no facts whatever. This is, however, not very surprising. Mr. Romanes is not a practised naturalist. His method is the very inverse of that of Mr. Darwin. We know that the latter for more than twenty years patiently accumulated facts, and then only reluctantly gave his conclusions to the world. Mr. Romanes, on the other hand, frames a theory which looks pretty enough on paper, and then, but not till then, looks about for facts to support it.

In my view, one is not called upon to give much attention at present to physiological selection. Still, a word or two may be devoted to it. The *Times* took an exception to the phrase of which I am surprised that Mr. Romanes has taken so far no notice. It says :—"How his theory can properly be termed one of selection he fails to make clear. If correct, it is a law or principle of operation rather than a process of selection." In point of fact, what Mr. Romanes calls physiological selection may be more accurately described as reproductive isolation. He supposes that individuals of a particular species arise which from some cause or other are incapable of breeding with other conspecific individuals. They are therefore in one aspect isolated, as if they were on an oceanic island. This being so, any casual variations which they exhibit will be perpetuated, he thinks, whether adaptive or not. And in this way he also thinks that species distinguished by non-adaptive characters have arisen. The idea is interesting, and Mr. Romanes believes that Mr. Darwin would have welcomed it. We know, however, that it occurred twelve years earlier to Mr. Belt, that Mr. Darwin was acquainted with it, and that "he did not regard it with any great favour." I myself have carefully considered it in connection with a variety of facts, and I have arrived at the conclusion that it is not a principle of very much value. It would take too long to set out the grounds for that conclusion here. But I may point out that such an isolated race would get no immunity from the general struggle for existence, while it would lose all the advantages to be obtained from free intercrossing. I am disposed to agree, then, with Mr. Wallace that, far from such races being "unable to escape the preserving agency of physiological selection," they would be very short-lived. Before leaving the subject, I cannot but remark on Mr. Romanes's singular choice of an alternative name for physiological selection—the "segregation of the fit." Segregation, I agree, is an improvement ; but "fit" lets in the whole train of adaptive ideas, while Mr. Romanes insists that "the variations on the occurrence of which it [physiological selection] depends are variations of an unuseful kind."

One remark, and I shall conclude all that I propose to say about Mr. Romanes and his theory. What I introduced into my Bath address I had had long before in my mind. While I was writing it, Prof. Huxley's obituary notice of Mr. Darwin came into my hands. I read it with the keenest pleasure, as everyone must; and I pointedly referred to it with a pardonable anxiety that a piece of work perhaps one of the most remarkable that ever came from that admirable literary workshop should attract a wider attention than from its mode of publication it might possibly receive. Personally, with regard to indifferent variations, I am a little disposed to think that Mr. Huxley is inclined to make too great concessions. I quite admit that correlated variation does give rise to a large class of non-significant characters. But I feel more and more that natural selection is a very hard taskmaster, and that it is down very sharply on structural details that cannot give an account of themselves. I doubt if there is much room in Nature for indifferent variations; and even correlated variations must be anchored, as it were, to an adaptive variation which has to bear the brunt of the maintenance of the whole correlated train.

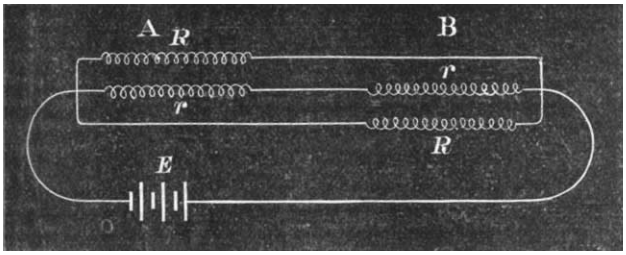
W. T. Thiselton Dyer.

Royal Gardens, Kew, October 26.

Electro-Calorimetry.

In a paper read at the British Association meeting at Bath, Messrs. Stroud and Haldane Gee describe the method used by them for heating the liquids under experiment. Will you allow me to point out that the series arrangement of the coils is electrically in unstable equilibrium, since any difference of temperature between the baths causes less power to be spent in the cooler one, thus tending to increase the difference. With the coils in parallel less power is spent in the hotter bath, but the method is still imperfect from the want of equality of heating at different temperatures.

Coils may, however, be so arranged as to completely overcome these defects in an otherwise very simple and convenient apparatus. In the figure let A and B represent the two baths and the coils therein, each bath being heated partly by a series coil of r_ω, partly by a parallel coil of R_ω. All four coils should be made of the same metal.



The necessary relation between R and r to secure equal heating may be found by writing $\frac{d \text{ watts in A}}{d\,\theta} = \frac{d \text{ watts in B}}{d\,\theta}$, where θ is the difference of temperature between the baths. When worked out this gives $R = 4r$; a result which is obviously true provided the coils have only a small temperature coefficient.

Sydney Evershed.

2 Victoria Mansions, S.W., October 10.

The "Tamarao" from Mindoro (Philippine Islands).

I have only just seen, in Nature of August 16 (p. 363), Dr. Sclater's communication of Prof. Steere's letter concerning the discovery of a new species of *Anoa* (*A. mindorensis*) in the Island of Mindoro. I beg to say that I forwarded a note on this imperfectly-known animal, whose native name is *Tamarao* (not "*Tamaron*," as far as I know), to the Zoological Society of London, and the note was printed in the Proceedings of the Society for 1878, pp. 881-82, under the title, "Letter concerning the supposed existence of the *Anoa* (*Anoa depressicornis*) in the Philippines." Since then, Dr. Hoffmann, formerly Assistant at the Royal Zoological Museum of Dresden, has published the results of his investigations on a skull of the *Tamarao*, which has belonged to the Dresden Museum since 1878, and which was brought by Prof. Semper from his travels in the Philippines (see *Abhandl. und Berichte d. k. Zool. und Anthr. Ethnogr. Museums zu Dresden*, 1886-87, No. 3, p. 26 *et seq.*, Plate 6, *a–f*). He proves, by a comparison of this skull with the skull of *Anoa* from Celebes, and with buffalo skulls from the Philippines and elsewhere, that this *Tamarao* has nothing to do with the genus *Anoa*, but is a true buffalo, viz. either *Bubalus indicus*, Rüt., or an undescribed variety of this species, or, perhaps, a new species of *Bubalus*. Between these alternatives we were unable to decide from the single skull in our hands, which, besides, is not that of a full-grown animal. If Prof. Steere be right in asserting that there exists a true *Anoa* in Mindoro, I can only conclude that the skull brought by Prof. Semper as that of the *Tamarao* of Mindoro, is not the true *Tamarao*.

R. Museum, Dresden, October 17. A. B. Meyer.

Pallas's Sand-Grouse (*Syrrhaptes paradoxus*).

It is obvious that this bird no longer appears to come much, if at all, under observation in Europe, although it was reported from almost every part during the months from April to June (see Meyer and Helm, *Orn. Jahresbericht der Beobachtung-stationen im Königreich Sachsen*, iii. p. 117 *et seq*), and even later. I suppose nearly all the specimens have flown into the sea, and been drowned there. As regards its former appearances in Europe, a specimen of *Syrrhaptes paradoxus* is said to have been killed near Grenzdorf, in Silesia, about four years ago; and it is also said to have been observed near Sagan, in Silesia, in the years 1874–78; and in the year 1883 near Münster, in Westphalia. Whether these reports are authentic, I, of course, cannot say, the specimens not being in my hands.

R. Museum, Dresden, October 17. A. B. Meyer.

The Species of Comatulæ.

The writer of the notice of vol. xxvi. of the *Challenger* series, which appeared in Nature of October 11 (p. 561), remarks that the total number of living species of Comatulæ is given on p. 383 as 180, but that from the distribution list itself there would seem to be 188 species, and he adds that "possibly the seven additional species of Antedon and the one species of Actinometra named but not described may account for this discrepancy." If he will look at the list again he will find that though it contains the names of 8 MS. species, three of them belong to Actinometra and only five to Antedon. These, however, do not account for the apparent discrepancy, which is due to the fact that eight species are dimorphic, so that their names appear twice over, as is fully explained in the systematic tables on pp. 54, 58.

It will, of course, be understood that these lists only contain the names of such species as have yet been baptized, some few having received names before they could be described, on account of their serving as hosts to Myzostomida, which have been reported on by Prof. von Graff. But some time must unfortunately yet elapse before it becomes possible to make out a complete systematic and distribution list of all the Comatula-species which are still awaiting description in various Continental Museums. Some very interesting forms were obtained by the German ship *Gazelle* and by the Italian cruiser *Vettor Pisani*. Prof. Semper's Philippine collection, which contains several unusually fine individuals, is as yet undescribed, and I know of many other new types from various localities. At present, however, the fine collections made by the *Blake* in the Caribbean Sea during the years 1877–79 are occupying most of my little working time, and they well repay investigation.

Eton College, October 26. P. Herbert Carpenter.

Voracity of the Haddock.

A small haddock (*Gadus æglefinus*), alive when purchased on the fish quay this morning, was so much distended that curiosity prompted an investigation of the cause. In the stomach were found fourteen young whiting (*G. merlangus*) from 4 to 5 inches long, and a small crab (*Carcinus mænas*), with hard carapace, about 1 inch in diameter, all quite fresh, and digestion barely commencing. The haddock was 17 inches long, and weighed, when gutted, 26 ounces. The weight of the young fry and crab was $6\frac{1}{2}$ ounces, or almost one quarter of the weight of the fish. Doubtless this record is often beaten in the deep, though the evidence of so healthy an appetite among fishes is not often so apparent.

Chas. O. Trechmann.

Hartlepool, October 30.